

The Quest for the Canon: Holy Grail or Empty Economic Box?

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This volume contains a selection of twelve papers presented at the Third European Conference on the History of Economics, held at Panteion University of Social and Political Sciences, Athens, 17-19 April 1997, together with an editorial introduction. Readers whose sources for the study of the history of economics are restricted mainly to authors resident in England or America will be surprised and delighted to find that all fifteen contributors (two of the essays are jointly authored) reside elsewhere – four in Greece, three in the Netherlands, two in Belgium, and one each in Brazil, Italy, Ireland, Austria, Norway and Switzerland – but, as a concession to monolingual anglophones, all the papers are in English.

The editor's Introduction 'The canon as the history of economics and its critique' (pp. xi-xvi) provides a succinct summary of the twelve papers and shows how the attention that economics is now giving to the nature and development of its canon is derived from similar investigations in literary criticism. He emphasises the current lack of consensus amongst economists about the content of the canon of economics, and sees these twelve essays as a contribution to a more precise definition. His own view is that 'the canon in the history of economics is not an obvious theoretical construct; it is itself a historically determined concept' (pp. xii-xiii) – a view that commands strong support from this reviewer also, but that raises doubts about the feasibility of identifying an economics canon capable of enjoying an objective existence independent of the whims and fancies and ideological prejudices of the person endeavouring to define it.

It is somewhat paradoxical that the first essay in this collection devoted to the so-called canon in the history of economics should begin with the following statement that emphasises (quite rightly) the fleeting and transitory nature of canons:

The history of ideas illustrates with ample evidence that canons, even the ones which enjoyed revivals, should better not be introduced and profiled as timeless universals. Like all human artifacts, even the most promising canons perish. Like all products of the human mind, they are up to a certain degree context-dependent and thus coded by the values, the norms, the paradigmatic modes of thought and the historical consciousness prevalent in the society from which they spring. Canons live a cycle of birth, growth and flourishing, followed by re-interpretation and decline. (p.1)

This statement – which opens Chapter 1, 'The Mediterranean trajectory of Aristotle's economic canon' by Louis Baeck (Emeritus Professor, Catholic University, Leuven, Belgium) – could almost be interpreted as a reason for the reader not to proceed any further. Attempts to define canons may help us to understand the times in which they flourished, but – in the absence of a modern

revival of an earlier canon, an event rendered unlikely by its ephemerality – does the identification of previous canons have any interest (other than antiquarian) for our current life and times? When we have found and agreed upon a canon, what do we do with it?

Professor Baeck, in a learned treatise that uncompromisingly poses difficulties for readers unfamiliar with Greek and Latin, shows how Aristotle's economic canon, as it moved through the Renaissance to modern times, was transformed into 'a completely different canon of unending progress, utilitarianism and welfare maximization, all this driven by self-interest' (p. 1). Economics as 'the queen of the social sciences ... would have been anathema to Aristotle' (p. 1). Baeck concludes, therefore, that 'Aristotle cannot be profiled as an ancestor or fundamental base of modern economics. As I see it, Aristotle should rather be seen as a radical critique of modernity's economization of society' (p. 22) – a view that presents a challenge to historians of economics who claim an unbroken line of ascent from Aristotle to the latest issue of the *Economic Journal*.

Chapter 2, 'The idea of usury in Patristic literature', by Thomas Moser (Researcher at the Institute for Business Cycle Research, Swiss Institute of Technology, Zurich), is another scholarly treatment of early economics. The title is a little misleading, as the article begins with the ideas on interest-taking in the Old Testament, Graeco-Roman literature, and the New Testament, before proceeding to Clement of Alexandria (c. 150-215); Tertullian (c. 160-220) of Carthage; Cyprian (c. 200-58), bishop of Carthage; the Council of Elvira in Spain, AD 306; the first Council of Arles, AD 314; the first general Council of Nicaea, AD 325; Lactantius (c. 256-325); Basil the Great (c. 330-79); Gregory of Nazianus (c. 329-90); Gregory of Nyssa (c. 335-94); Ambrose of Milan (c. 339-97), bishop of Milan; John Chrysostom (c. 350-407), patriarch of Constantinople; Jerome (c. 347-420); Augustine (c. 354-430); and finally to various canons promulgated in the seventh, eighth and ninth centuries.

Moser persuasively argues that 'In the history of ideas, there are only a few questions that have occupied human minds longer than the question of the justification of interest taking', but that although histories of economics deal with Greek and medieval ideas on interest, 'Patristic literature rarely receives the attention it deserves' (p. 24). According to Moser, it was the Fathers of the Church who selected and transmitted Biblical texts to the medieval writers and whose commentaries greatly influenced medieval Christian tradition on interest, as on other matters.

Chapter 3 deals with 'Self-interest as an acceptable mode of human behaviour', written by Professor Arild Saether (Department of Economics, Agder University College, Kristiansand, Norway). Referring to Adam Smith's idea that the pursuit of self-interest promotes the good of society, he disputes the view that this idea was 'of indigenous Anglo-Saxon growth and owed little, if anything, to outside sources' (p. 45). He argues on the contrary that Smith was influenced by Grotius and Hobbes and particularly by Pufendorf, whose ideas came to Smith via Gershom Carmichael and Francis Hutcheson. Saether believes that Pufendorf, by formulating a synthesis of Grotius and Hobbes, showed that man's self-interest, when checked by man's sociability (i.e. his inclination to live in society), is 'the driving force behind human behaviour' and leads to the best society. Smith adopted this view, together with Pufendorf's view that man does not possess an innate moral sense, but has to be educated to live in society.

Reactions of readers to Chapter 4, 'Deconstructing the canonical view on Adam Smith' (pp. 68-91) by Jan Peil (Department of Applied Economics, Catholic University of Nijmegen, The Netherlands) will depend (at least in part) on whether they are familiar with, and sympathetic towards, the vocabulary of linguistic analysis. They might puzzle, for example, over the following definition of 'dialogue': 'For, in contrast to the monologue, the dialogue is attuned to generating and legitimizing meaning and knowledge in the intersubjectivity of the communication with someone else' (p. 78). They might wonder whether this definition of 'dialogue' means anything more or less than 'two people talking'; or whether they should in future abandon the term 'dialogue' in favour of 'the intersubjective concurrence of dialogical communication' (p. 80).

Apart from these linguistic difficulties, a more substantial concern is the identification of 'the canonical view on Adam Smith'. Obviously, before the canonical view can be deconstructed, it must be defined. Jan Peil states that Schumpeter's general equilibrium interpretation of Adam Smith is 'the canonical interpretation of the last forty years' (p. 71), but no evidence is presented in support of this questionable statement. Schumpeter's general equilibrium interpretation of Adam Smith could be seen as yet another example of his lifelong attempt to prove that economics is a positive science akin to physics. Some critics would regard his attempt as a pretentious delusion, and would reject the claim that Schumpeter's interpretation has become the canonical view of Smith, pointing instead to the many interpretations that stress the political, ethical and institutional dimensions of Smith's work.

To avoid disputes about what is or is not the canonical view on Adam Smith, Peil's article could have been entitled 'Deconstructing Schumpeter's view on Adam Smith', and would have achieved its aim admirably. It shows clearly that according to Smith the wealth of nations will depend not on a merely mechanistic manipulation of production functions, but on an appropriate assemblage of psychological, ethical, and political attitudes.

Chapter 5, 'The "canonical" model of economic growth in the debate between Ricardo and Malthus' (pp. 92-109) by Terenzio Maccabelli (Research Assistant, University of Parma, Italy) is a very interesting and successful attempt to show that, even if Ricardo's growth model provides the basis for Samuelson's 'classical canonical model of political economy', Malthus' growth model differs from Ricardo's in some essential respects and cannot be subsumed into Samuelson's canon.

Employing Sen's distinction between the engineering and the ethical approaches to economics, and the distinction between closed models that are restricted to formalised, axiomatic, mathematical and deterministic sequences, and open models that incorporate cultural, psychological and anthropological variables such as tastes, motives, and social institutions, Maccabelli argues that Malthus' stress on the role of motives creates a major difference between his 'open' theory and Ricardo's 'closed' or canonical theory. He therefore rejects (p. 99) the view of Samuel Hollander and others that Malthus' model of economic growth is the same as Ricardo's. Maccabelli provides convincing textual evidence to show the importance that Malthus attached to psychological factors that promote or inhibit growth, such as the existence of a taste for luxuries and the desire for indolence and ease.

Another motive – not denied by Maccabelli but perhaps not given the emphasis that Malthus gave to it – is the motive of profit prospects, arising from the

increased effective demand generated by a wider distribution of income and wealth. For Malthus, motivation for investment and production was as important as motivation for consumption.

Chapter 6, 'In defence of a traditional canon: a comparison of Ricardo and Rau' (pp. 110-126) by Peter Rosner (Professor of Economics, University of Vienna, Austria) investigates 'the reasons for giving Ricardo a more prominent place in the pantheon of economists than Rau' (p. 112), and in so doing pursues more general questions such as: should we abandon attempts to establish a canon and instead be content to study the great variety of contributors, minor as well as major, to the history of economics? and if we decide to define a canon, should it be the traditional canon or a modern canon based on the contributions of recent decades? Underlying this study, there is the conviction that a mainstream or core of economics does in fact exist today – as evidenced by the similarity all over the world of economics curricula, textbooks, technical language, etc.

As Ricardo's views are widely known, Rosner sensibly devotes more space to an exposition of Rau's views. Readers less familiar with Rau will be surprised and fascinated by the range and cogency of his ideas. Some readers might be led to conclude that here is a writer far more interesting and relevant than Ricardo. They may even want to conclude that the fact that Ricardo is given a more prominent place than Rau in the mainstream canon is more an indictment of the mainstream canon than a criticism of Rau.

However, the thesis forcefully presented in this chapter gives rise to the problem of reconciling truth and logic in Ricardo. Rosner describes Ricardo's theory as 'elegant' and 'systematic' and states that his 'equivalence theorem, his theory of rent, and the theorem of comparative costs were systematically argued within a theory developed by Ricardo himself. They are logical conclusions' (p. 124). He also argues that 'the theoretical basis of Ricardo' is his labour theory of value (p. 124), but adds that 'today most economists would reject not only a labour theory of value but any purely cost-based theory' (p. 124). He justifies Ricardo's eminent place in the mainstream because of Ricardo's 'contribution to current knowledge', where 'knowledge' is described as 'the set of research strategies, theories and empirical propositions which are *considered to be true*' (p. 113; emphasis added).

If the mainstream canon is defined, not as a set of theories 'considered to be true' but as a set of theories considered to be logical – i.e. logically derived from a basic premise – then the conventional belief of the majority of historians of economics that Ricardo was very logical provides sufficient justification for his occupation of a prominent position in the canon – the only reservation being the possibility that in this, as in some other instances in the history of economics, the majority might be mistaken.

But if truth, rather than logic, is the criterion for including theories in the mainstream canon, what justification can there be for including conclusions derived (logically) from false premises? Ricardo's basic premise – the labour theory of value – is considered to be false, but mainstream theories derived (logically) from it are 'considered to be true'. Is it possible for true conclusions to be drawn logically from false premises?

Theories could be classified into four categories: (i) those that are logical but not true; (ii) those that are true but not logical; (iii) those that are both logical and true; (iv) those that are neither logical nor true. It would be a strange mainstream that either included category (iv) theories, or excluded category (iii)

theories. If Rosner is correct in the case of Ricardo, it would seem that the mainstream of economics is prepared to accept category (i) theories but not category (ii) theories.

There could of course be reasons other than truth and logic for the existence and persistence of a mainstream core. For example, a particular set of theories and techniques can become established as a hegemonic canon for reasons of curriculum inertia. 'A taste for indolence and the love of ease' (to use Malthus's phrase) can explain the reluctance of academics to discard lectures painstakingly developed during a professional lifetime. Peer pressure and the fear of losing status and students can explain why any one university is afraid to deviate from the norm; we have to teach mainstream economics because everyone else does. Paradigmatic censorship – the control exercised by our professional elders over appointments, promotions, research grants, and publications – can exert a powerful influence over the thoughts of junior staff. And, as some critics have cynically suggested, universities and academics will tend to receive financial and other rewards in proportion to their adherence to a canon that is consistent with the views of the dominant political party. The power of these psychological and sociological forces in creating and fostering a particular canon should not be underestimated.

Chapter 7 is 'Cracking the canon: William Stanley Jevons and the deconstruction of "Ricardo"' (pp. 127-145) by Bert Mosselmans (Research and Teaching Assistant, Free University of Brussels, Belgium). It describes the Ricardo-Mill or classical canon as 'a reproductive system with internal scarcity', and the Jevons or neo-classical system as one of 'non-reproductivity and external scarcity'. In the reproductive system, scarcity results from the 'misbehaviour of certain individuals (and therefore can be avoided by an alteration of behaviour)' (p. 125). An example of this bad but remediable behaviour is 'ignorant men putting unmaintainable beings into existence' (p. 129) – behaviour that Malthus described as immoral.

The non-reproductive system devised by Jevons is said to be a 'mechanical system governed by outward causes' (p. 128). In this system scarcity arises from external causes and 'cannot be avoided by an alteration of human behaviour' (p. 128). An example of an external scarcity is the scarcity of coal in England, as discussed by Jevons in *The Coal Question*. Mosselmans also argues that Jevons, in formulating a non-reproductive, non-harmonious view of the economy, was influenced by two external shocks – the railway crisis of 1847-48 in which his father and other family members became bankrupt, and the death of his father in 1865.

This reproductive/non-reproductive taxonomy is novel and interesting, but would appear to require some further refining if it is to become a useful tool of analysis. For example, it could be argued that the railway crisis of 1847-48 was not a shock external to the system, but was an internally-generated phenomenon – viz. the oscillatory boom-and-bust characteristic of nineteenth-century capitalism – and that it was caused by 'the misbehaviour of certain individuals' – viz. rash over-investment by banks and speculative entrepreneurs, like Jevons's father. More generally, the dividing line that distinguishes internal from external causes of scarcity is difficult (impossible?) to draw in a general-equilibrium framework that is truly general – i.e. where everything is influenced by everything else. To characterise Jevons's system as 'non-reproductive with external scarcity' on the basis of just one of his works (*The Coal Question*) might also be considered rather

unwarranted, as indeed would the extension of this categorisation to neo-classical economics as a whole.

The reproductive/non-reproductive dichotomy becomes even more tenuous when it is argued (a) that Ricardo's stationary-state model belongs to the former category because the stationary state remains reproductive – 'the economy is reproduced on the same level' (p. 130). Did the Jevons and the neo-classical models predict economic annihilation? and (b) that in Ricardo's system 'ultimate scarcity can be avoided through the adoption of free trade; and, in this sense, guilt is laid upon persons unwilling to pursue this policy' (p. 129). This seems to imply that anyone who opposes Ricardo and free trade is not only stupid, but also guilty of misbehaviour and immorality.

In Chapter 8, Terrence McDonough (Lecturer in Economics, National University of Ireland, Galway, Ireland) deals with 'Who blushes at the name: John Kells Ingram and minor literature' (pp. 146-155). McDonough notes that textbooks in the history of economic thought usually select the same groups of authors for intensive treatment, and that the question of why some are included in the canon and others excluded is rarely addressed. In this chapter McDonough looks at the latter part of the question, i.e. what is it in an author or his works that makes him unfit for inclusion and deprives him of canonicity. The Irish historical economist, John Kells Ingram, is examined and the conclusion reached that his non-canoncity is the result principally of his 'stubborn refusal to constitute economics as a separate science' (p. 151). According to Ingram, economics should be seen as one chapter in the great science of society – 'Sociology' – and should be studied in close relation to the rest of Sociology.

McDonough provides us with an interesting counter-factual hypothesis. What would the canon of economics be like if Ingram was one of its central figures? In such a canonical revision, standard h.e.t. textbooks would tell us that Carl Menger, instead of being heralded as a founder of a new, Austrian school, would be described (in Ingram's words) as representing a 'movement of reaction in favour of the older school'. The revised canon would also assert (in Ingram's words) that 'the application of mathematics in the higher sense of economics must necessarily fail', and that Jevons's 'theoretical constructions' are nothing more than 'academic playthings' (p. 153; quoted from Ingram, *A History of Political Economy*, 1888, pp. 221, 233-34).

Thus Chapter 8 cleverly reminds us that the so-called canon of economics is not a unique, homogeneous entity, but that there are as many canons as there are diverse schools. Because Samuelson's seminal 1978 article was concerned with the canonical classical model, there is perhaps an unfortunate tendency in the subsequent literature to concentrate exclusively on the canon of classical economics, forgetting that the institutional school, the Marxist school, the Keynesian school, etc. also have their canons; and – assuming that the search for canons is a useful endeavour, an assumption to be queried below – these other canons are as worthy of being investigated as the canon of classical economics.

There is one point, however, in McDonough's stimulating essay on which the reader might be inclined to take issue. It is said that 'the key' to Ingram's non-canoncity is his 'refusal to designate economics as a separate science' (p. 152). A contrary opinion might be that a refusal of scientific separatism was not the specific reason for his non-canoncity. The 'key' to Ingram's non-canoncity would appear to be his institutionalism and his anti-neoclassicism, rather than his unwillingness to accept economics as a separate science. Even if he had regarded his brand of

economics as a separate science, he would have been denied entry to the holy of holies of classical economics.

Chapter 9 is written by Mauro Boianovsky (Professor of Economics, Department of Economics, Universidade de Brazilia, Brazil) and is entitled 'In search of a canonical history of macroeconomics in the interwar period: Haberler's *Prosperity and Depression* revisited' (pp. 156-179). Gottfried Haberler (1900-1995) published in 1937 his *Prosperity and Depression: A Theoretical Analysis of Cyclical Movements*. This work has subsequently been acclaimed as an authoritative history of the development of pre-Keynesian business cycle theory, but Boianovsky points out that although Haberler gave us 'a comprehensive and critical assessment of the complex inter-war macroeconomics' (p. 173), and although Haberler's classification scheme for inter-war business cycle theories can usefully be applied to other periods, there are few historical references in the book and Haberler himself explicitly stated that it was not intended as a definitive history of business cycle theories.

Chapter 10, 'Preobrazhensky and the theory of economic development' (pp. 180-195), by Michalis Hatziprokopiou and Kostas Velentzas (Professor and Associate Professor respectively, Department of Economics, University of Macedonia, Thessaloniki, Greece), argues that Evgeny Preobrazhensky (1886-1937) put forward a theory of industrial development that can be applied not only to socialist countries, but also to less developed countries in general, and 'can be incorporated without any difficulty in the main body of mainstream economics' (p. 192). In his theory, the capital necessary for industrialisation would be extracted from the agricultural sector, not by forced collectivisation, but by a price policy that regulated the internal terms of trade (between industrial products and agricultural products) without the need for foreign investment. The authors argue that Preobrazhensky should therefore be recognised as a pioneer of development economics.

If they are correct in their assessment, the relative neglect of Preobrazhensky by mainstream economics shows how membership of a recognised canon can sometimes be unfairly denied to deserving individuals, and how, despite its honorific implications, a canon as currently conceived may be deficient in its coverage of persons and ideas, and in need of constant revision and re-definition.

In Chapter 11, 'Canon and heresy: religion as a way of telling the story of economics' (pp. 196-219), Albert Arouh (Professor of Economics, American College of Greece-Deree College, Athens, Greece) argues that the history of economics can be described by using religion as a metaphor, and by showing how conflicts in economics between believers in the dominant paradigm and dissenters can be compared to similar conflicts in the history of religion. He sees the history of conflict in economics as 'a conflict between a religious canon and a heresy which strives to reform it. It [is] a story of the battle between two dogmas fighting for power' (p. 212).

Professor Arouh acknowledges that his comparison between economics and religion is not new. He quotes similar comments from a number of writers – such as Knut Wicksell, Frank Hahn, Phyllis Deane, and particularly Guy Routh (p. 203). But there appears to be an important difference between Arouh's position and that of his cited sources. Whereas the latter describe the dominant orthodox position as a matter of faith and as immutable, undisputed, intransigent dogma, Arouh seems to suggest that the heresy that seeks to replace the canon will be equally devoid of rational foundation. He confidently announces as an absolute truth, and one not

requiring justification, that ‘there is no absolute, i.e. externalist, rationality by which to choose between fundamental beliefs in economics’ (p. 200). This seems to say that in economics the only absolute truth is that there are no absolute truths.

The old-fashioned notion that economists are engaged in a search for ‘truth’ – i.e. a search for theories that best explain economic reality and for policies that best promote economic welfare – seems quaintly naive by comparison with the post-modern new. If indeed this popular relativistic position, bordering on economic nihilism, is the only intellectually-tenable position, economists should accept that their ‘science’ is no more than a party game, an entertaining diversion, or a trivial pursuit. What used to be called ‘classical economics’ and ‘neoclassical economics’ should hereafter be called ‘Nintendo economics’.

The economics-as-religion analogy, so usefully employed by Professor Arouh, lends itself to extended applications. Who is the current Pope of neoclassical economics, and where is the Holy See? The University of Chicago would be a strong contender for that honour. How does the Neoclassical Church conduct its Inquisition? Thumb-screws and racks are rather crude means of suppressing dissent, but the controls exercised by the leaders of the scientific community over appointments, publications and promotions are just as effective in protecting the paradigms of normal science. The religion of neoclassicism is also as proficient as any other religion in matters of ritualistic observances and mysterious languages. The incantation of shibboleths and formulas such as ISLM, $MR = MC$, and $AS = AD$, brings peace and contentment to the minds of the faithful.

The final chapter, Chapter 12, written by Henk W. Plasmeijer and Evert School (hereafter, P & S), is entitled: ‘The neo-classical synthesis in the Netherlands: a demand and supply analysis’ (p. 220-44). The authors are, respectively, Associate Professor at the Groningen State University, and Director of Graduate Studies at the Research School SOM of the University of Groningen, the Netherlands. It contains some interesting statistics for the Netherlands on the employment of economists, the number of economics graduates, the number of economics students in universities, and the subject matter of articles in two leading economics journals. Unfortunately, the statistics do not go beyond the 1970s: recent figures would have been even more interesting. From this Dutch experience, the authors make a number of insightful observations on the nature of canons in the history of economics in general, and on the development of the canon of economics in the Netherlands in particular.

They argue that Dutch economists have been characterised in the past by a policy orientation (rather than a theoretical orientation) (p. 221). They point out that this policy orientation developed in the early 1930s as a reaction against the Dutch Austrian school. Dutch economists became aware that they had become ‘participants in social policy’ and that ‘the Pontius Pilatus trick did not work any more’ (p. 235).

However, P & S argue that ‘There is no reason why this profile could not change in the near future’ (p. 240). They are of the opinion that ‘nowadays the policy orientedness of the economists is – as everywhere on the continent – to an increasing extent seen as a hindrance to academic success’ (p. 222). They argue that in the 1980s the orientation has been towards microeconomics, and that ‘We are witnessing a “European” process of an ever more narrowing academic profile’ (p. 222) – a view that readers might wish to extend beyond Europe. In their view the *tâtonnement* process which had previously produced a supply of Dutch policy-oriented economists might now start to generate a supply of Dutch theoreticians.

With due loyalty to the ideal of value-free economists, P & S do not express a value judgement on the merits or otherwise of such a change in the profile of Dutch economics. Nor do they venture an opinion on the expected life-span of this recent trend towards theoretical microeconomics. If canons tend to be not immutable, but fleeting and transitory (as Louis Baeck argued in Chapter 1), and if they are also historically determined (as Michalis Psalidopoulos argued in the Introduction), how long will it be before policy-orientedness will again take over as a revolt against theory-orientedness? The revolt will be hastened if at least some Dutch (and hopefully other) economists retain a vestige of their commendable policy-orientedness and begin to put to their theoretical, canon-worshipping counterparts questions such as: When you find your missing canon, what will you do with it? *Cui bono*? What is the use of it? Will it turn out to be anything more than a set of trite truisms, dressed up with the pretentious title of 'Laws'?

P & S seek to establish a distinction between a canon and its 'social functioning'. They argue that 'It goes without saying that no such thing as a European or American science can exist. What may differ is the social functioning of the canon, for good as for bad' (p. 222).

This distinction between a science and its social functioning might well be valid in the case of the physical sciences and mathematics. The notion that there is, for example, a distinctively European or American mathematics is hardly credible; but there is no doubt that at different times and places mathematics and physics can be used, and have been used, to perform widely different social functions – to conduct war or to promote peace being an obvious difference. But one wonders whether the distinction drawn by P & S between a science and its social functioning is sustainable in the case of a social science. Is this a case of scholastic hair-splitting, an attempt to create a distinction where there is no difference? For economics, the answer will depend on whether, despite being a social science, it can claim to be a hard, positive, value-free science, like mathematics or physics. Dutch economics appears to have been strongly influenced on this issue by P. Hennipman, who 'fought the battle for a value free economics' and 'explicitly denied that normative statements are involved in welfare economics' (p. 237). According to P & S, 'For more than one generation of Dutch economists, Hennipman's essays epitomized the creed of value free economics, meticulously documented from the entire history of economic thought' (p. 237).

However, although the authors suggest that the *tâtonnement* process changed the profile of Dutch economics from a theoretical orientation to a policy orientation, they do not extend their suggestion to the question of value-free *versus* value-laden economics. Despite the ever-increasing volume of international disaffection with the concept of a value-free economics, they do not cite any modern Dutch rejections of the Hennipman thesis, and do not suggest that the same *tâtonnement* process might one day produce a generation of Dutch (and other) economists convinced that, on the question of their ethical and/or ideological predispositions, as previously on the question of policy-orientation *versus* theory, they can no longer perform 'the Pontius Pilatus trick'.

Although the twelve chapters of this book contain much that is both interesting and likely to stimulate lively controversy, one notable theme is missing. None of the authors has seriously questioned either the usefulness of the quest for the canon of economics or even the existence of a canon in any meaningful sense.

It could be argued, for example, that in studying the history of economics it is more interesting and more important to celebrate the differences between

economists than to attempt to identify what they have in common. Perhaps it is through the variety of opinions rather than their similarity that economics is more likely to progress. Evolution is said to be characterised by increasing diversity, rather than increasing sameness.

The search for a canon could even prove to be an impediment to progress in economics, inasmuch as it becomes a straitjacket that inhibits the movement of research into the immense variety of lesser-known but possibly fruitful sources, or inasmuch as it becomes a novel, modish pastime that diverts attention away from pressing real-world issues and into a mythical world of ‘the substance-hiding-behind-the-accidents’ – a substance that may well prove to be an empty economic box or a box that is self-referentially found to contain precisely what each searcher hoped it would contain. One cannot escape from the very real possibility that the search for the so-called canon is not an objective, academic exercise, but is perhaps motivated, consciously or otherwise, by vested interests, whether it be the economic self-interest of individuals and organisations who use the canon to justify their accumulation of personal fortunes; or whether it be the professional self-interest of academics who derive job satisfaction, personal fulfilment and a sense of righteousness from the knowledge that their ideas belong to the canon, and who enunciate the self-styled ‘laws’ of their canon with the kind of reverential awe of crusaders who have discovered the Holy Grail, disparagingly dismissing all non-canonical views as heresies. Is the quest for the canon an exercise in hagiography and canonisation, for the current disciples as well as the founders? It is perhaps significant that attention is being devoted to identifying the canon or core of economics at a time when economics as a whole is increasingly under threat and falling into disrepute. Is the quest for the canon a last-ditch attempt by the ultra-orthodox to close ranks and to guard their most precious icons – diminishing returns, marginal productivity, comparative advantage, supply-and-demand geometry, etc. – against the depredations of the anti-neoclassical barbarians?

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